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‘I have never held models as depictions of anything real; they are just tools for understanding some aspects of the real world’



Interview with Amitava K. Dutt

Amitava Krishna Dutt is Professor of Economics and Political Science in the Department of Political Science at the University of Notre Dame, IN, USA, and Distinguished Professor at the Facultad Latinoamericana de Ciencias Sociales (FLACSO), Quito, Ecuador. He was educated at Presidency College, Calcutta, the University of Calcutta, India and the Massachusetts Institute of Technology (MIT), Cambridge, MA, USA. His areas of specialization are macroeconomic theory, development economics, international economics, and political economy. His current research focuses on global uneven development and globalization, models of growth and distribution, consumption and happiness, and the political economy of inequality. He is one of the co-editors of the *Review of Social Economy* and of *Metroeconomica*.

How did you become an economist and how did you become a heterodox or post-Keynesian economist, in particular?

In India, when students do well in school, they are often pushed to study either physics or economics. To study physics, I had to study biology, too; this involved dissecting animals, which I didn't want to do. So, by default it was economics! A youthful rationalization was also to reduce suffering in the world. I got my first degree as an undergraduate from Presidency College Calcutta which had rich traditions of many well-known people who studied there. The theory I was taught was completely mainstream: for microeconomics it was utility maximization and profit maximization and a whole lot of calculus, and for macroeconomics it was Neoclassical Synthesis Keynesianism. But then there were courses on the Indian economy and on economic history, and a complete mismatch between these and the theory courses. For instance, one of the very important issues discussed in the Indian economy courses is how inequality is bad for the economy. And you could see around you how bad the economy was, people living in the streets in very poor conditions. Hence, there was the theory on the one hand and there was the real world on the other. The idea that they can be put together came from one of our professors who was teaching a course on agricultural development in India and used a paper by Amit Bhaduri (1973) on semi-feudalism in agriculture. In retrospect, this was very crucial for showing me that one can have theories that can actually help in understanding the real world. Then, I also did a Master's degree in India which was very interesting. We had a former student of Solow's, Asim Dasgupta, who taught about growth theory, and about the Pasinetti paradox, because Solow had been interested in that. In addition, we had another teacher who taught

about Sraffa, and a history of economic thought course that covered Smith, Ricardo, and Marx. So, although most of the theory I learnt in India was neoclassical orthodoxy, I was also introduced to other things, though not in a very systematic way.

When did you get your Indian Master's degree and where did you go next?

I got my Master's degree in 1977 and then I moved to MIT in 1978.

Why MIT?

Well, Indians are very rank-conscious and MIT was number one at that time among economics graduate programs. And then Paul Samuelson, Robert Solow, and Evsey Domar were at MIT and, as I mentioned earlier, a teacher of mine had studied there and he encouraged me as well.

Was Lance Taylor one of the teachers, too?

Yes, he taught development economics at the time. And he was still doing planning models and dynamic optimization, but social accounting matrices as well. He had recently visited Brazil and Chile, I believe, and he was slowly becoming more heterodox, beginning to put class issues and quantity adjustments into his models. We touched upon the puzzle why inequality can lead to stagnation. Almost all models then suggested that high inequality led to high growth, which was not convincing to me. But anyway, Lance got me into heterodox economics. I had read a little bit before, but not much. So, I read Kalecki, Steindl, Kaldor, Robinson, Sylos Labini, Sraffa again, and a lot of other things. Lance Taylor had a course about this, though not many students took it. I also attended some lectures by Stephen Marglin, which later became the basis of Marglin's (1984) book.

Taylor and Marglin influenced you most when you were at MIT? Were there further professors who had an impact on you?

Marglin I barely saw, but his alternative frameworks approach had a deep impact on me. Lance Taylor was the major influence, without which I would probably still be playing optimization games. Paul Krugman was also on my PhD committee. And he said, 'You're doing this stuff and it's not optimization, you'll have trouble in the profession.' He knew Lance did similar things; and because they had done together the first paper that Krugman ever wrote, he had some regard for Lance, but he warned me. My thesis work followed Lance's structuralist approach. In fact the first paper that I wrote for my thesis was what eventually became the stagnation and income distribution paper (Dutt 1984). Then Lance came back from a long break, and he said, 'You know, I wrote a book that's based partly on your paper' (Taylor 1983). That felt good. However, Samuelson, Solow, Domar, Charles Kindleberger, and Richard Eckaus were also influences.

So in which year did you finish your dissertation?

I finished in 1982 and that 1984 paper was part of the dissertation. There were also two other papers. One was on food stocks and prices, and speculation, and it looked at how that affects development (Dutt 1986). In fact, Lance advised me to present that paper for my job talks, perhaps because it was theoretically somewhat sophisticated. But it is not that paper, but the 1984 paper, that is more widely cited.

When did you realize that it wouldn't be so easy to get a position despite the fact that you had been a graduate from MIT?

Well, I really didn't want to stay in the United States for long. I went to Florida International University where I stayed from 1982 to 1988. That was a blessing in disguise because with the work that I was doing I knew I wouldn't get tenure at a 'top' department – and here I could do whatever I wanted. I also had long discussions with a heterodox ex-physicist colleague, Panos Liossatos. It was great to have him around. In 1987 I got a call from the University of Notre Dame. They wanted somebody who would be formal but still heterodox, and so they asked Lance and, apparently, Lance had recommended me.

What about your work in this period, I mean the papers which grew out of your dissertation work and then led to the book in 1990?

That book was heavily influenced by Marglin's (1984) book and the idea of alternative model closures. I had a short paper criticizing Marglin for assuming full-capacity utilization (Dutt 1987), and that helped me to understand different models better. I was also working on North–South and uneven development issues, on technological change and money, so that all came together in the book (Dutt 1990).

When did you become aware that Robert Rowthorn in 1981 had published a model that was similar to your 1984 paper?

Actually, Edward Amadeo, who was doing a PhD at Harvard and also working with Lance, and whom I had not met before (but I worked with him later), wrote to me about some things and also told me by the way that there is this paper by Robert Rowthorn in an obscure journal and in the *Thames Papers in Political Economy*, and said it was mostly on the history of economic thought. I didn't have the paper. So I asked him to send me a copy, and then I felt, gee, somebody else also did what I had done; the differences were minor. But then I thought, well, it's good because it means I cannot be on the completely wrong track! When I met Rowthorn a few years later, we confirmed to each other that these were independent discoveries. But then you [Marc Lavoie] sent me the paper by Del Monte (1975), which I could never read because it is in Italian. He too, had discovered some of this before. Well, you know, nobody owns a model, right? I always called it the Kalecki–Steindl model, after those who were there even before us.

If we go back a little bit, you said that you had taken two or three lectures by Marglin but then you also responded to Marglin's (1984) book in a paper in the Cambridge Journal of Economics.

Yes, I argued that Marglin's analysis is incomplete because he missed the Kalecki–Steindl model with excess capacity, the only model that showed what we now call wage-led growth. Then Bhaduri and Marglin's (1990) paper came out, claiming that my (and Rowthorn's) Kalecki–Steindl model was problematic. And so, these two economists, who inspired me in many ways, were saying that my paper was wrong! I had to convince myself that it's not wrong, but different. To me the importance of wage-led growth is not really that increasing the wage share does necessarily stimulate growth but to show that this is a possibility that had been long ignored in formal models.

If we take the results of all this empirical work on wage-led or profit-led demand and growth seriously, usually profit-led growth comes in through the net-export channel. What is your take on that? And what are the economic policy implications from your work?

My 1984 paper focused on what several Indian economists – such as my former teacher, Amiya Bagchi – were arguing, that industrial concentration was responsible for high prices, low real wages, and a high mark-up. In fact, reducing concentration would improve

competitiveness, because prices would be lower. But anyway, not all places are the same – for some, real wage increases can reduce international competitiveness – and I was just interested in the logical possibility of reducing the profit share and improving competitiveness and net exports, which applied to India perhaps. Who knows if a particular economy is wage- or profit-led? The investment function is so dependent on expectations! When there is high inequality, it ought to be reduced. If that has an adverse effect (for instance, on growth), other policy steps can be taken. Political economy can prevent these changes. But these models may persuade people, including the powerful, that they can also gain from less inequality.

What about the applicability of your 1984 model and models that were similar to it? Do you think that they apply more to developing countries or to industrialized countries?

You know that Kalecki said that the problems of less developed countries have nothing to do with effective demand. It was all about inflation and food prices and things of that sort; it had nothing to do with his work on developed capitalist economies. But I think that semi-industrialized countries, with manufacturing and large service sectors, can have monopoly pricing and excess capacity, as in more mature economies. In fact, the high underemployment rates and uncertainty, fragmented markets, and institutional weaknesses in these semi-industrialized countries make aggregate demand very relevant.

But the people who object to these demand-led models for less developed countries usually argue on the basis of the underemployment of labor, not excess capital.

But service sectors have no real capital constraints and most of the estimates for manufacturing in India were showing low rates of capacity utilization. Although this could be due to other causes, the lack of effective demand due to high inequality is an important possibility. Furthermore, the channels which make an economy wage- or profit-led are net exports and investment. If you increase wages in the informal sector, which don't produce much of traded goods, there need be no negative effect on exports at all or on formal sector fixed investment.

In fact, your point of view is quite the opposite of that of Arslan Razmi (2015).

Arslan assumes in his work, like a lot of orthodox people, that small open economies, whatever that means, do not face a demand constraint. They are supposed to be price-takers, so they can export any amount they want. Ask exporters from less developed countries: can they do that? The answer will be 'no' for most manufactured goods! But Razmi assumes that, which yields full-capacity utilization and then gets profit-led results. No surprise! He misses the point that there are external demand constraints of various kinds, including marketing networks and quality perceptions.

Along with some colleagues, you developed what you called analytical political economy, creating a series of workshops. When did those start?

Well, the first one was held, I believe, in 1991 at Notre Dame and that was in response to the perception that heterodox economics has to be anti-mathematical. And to this day, I don't understand that claim. I have never held models as depictions of anything real; they are just tools for understanding some aspects of the real world. There were additional meetings at other places for a few years. Then it fizzled out after two edited volumes were published. A new beginning was made in 2007, I believe, with two of those involved in the earlier edition, Peter Skott and I, and some new people, such as Roberto Veneziani, who was particularly enthusiastic about it and organized a workshop at Queen Mary,

University of London, along with his colleague Simon Mohun. So, also together with Gil Skillman, we've been doing these workshops annually ever since then, with the five of us usually attending and inviting a number of selected other scholars each year. Our main purpose has been to promote rigorous analysis along heterodox lines. Another goal is to support young researchers by providing them a forum in which they can be encouraged and receive comments, allowing them possibly to improve their work and make it ready for journal publication, and thereby – we hope – assist them to get tenure and promotion.

After your 1990 book, what was next in terms of extending the work you'd been doing or discovering new fields?

Well, then I got more interested in the history of economic thought, again trying to formalize some of these things, for example Steindl, Malthus, and some Ricardian models. I continued working on a variety of models of growth and distribution, incorporating many features, such as conflict inflation, government debt, wealth distribution, into them and examining North–South uneven development (Dutt 2017). The one issue that drove me all along was inequality, whether it be international inequality or inequality within countries. I am glad to see now that so many people are interested in inequality. I also started working on the meanings and determinants of happiness (in a broad sense, as in Dutt 2008), ethics and political economy, and the importance of changing people, not just policies. This last issue was central to the ideas of Mahatma Gandhi (on whom, and for a discussion of other Indian economists as examples of non-Western contributors, see Dutt 2018).

Although you are a highly respected, leading figure in post-Keynesian economics, at some stage this was no longer appreciated at your university, the University of Notre Dame, and you, and others, were driven out of the economics department. How do you feel about this?

Those were difficult and painful times which I would prefer to forget, apart from what the episode implies about the fate of heterodox approaches and pluralism. I have now ended up in the political science department, which is much more pluralistic than the current economics department there. That in fact led me further towards political economy, focusing much more on power, and also international political economy and global power imbalances.

But dealing with power in economics, is it possible to go beyond the kind of work that was done by John Kenneth Galbraith? How much can one say or model?

Galbraith was important, but there were many other important contributors inside and outside economics. Also, Galbraith's work didn't have a formal structure; he raised issues about what power depends on but didn't link it up explicitly with distribution and growth. So, I'm working on those issues now: how power affects distribution and growth, and how those feed back to power (Dutt 2015). As Foucault argued, power is everywhere; so, it's relevant for analysing almost all of what we call social, political, and economic phenomena.

At Notre Dame, who has been closest to you among your colleagues?

I had several close colleagues, but the closest was Jaime Ros, who unfortunately passed away recently. I could discuss development theory with him and he was a wonderful person. We had lunch together every week, discussing many ideas. We edited books and wrote papers together. Philip Mirowski was another close colleague. Now, with Ben Radcliff, I work on happiness.

What is the lesson to be learned from your experience in this pluralist department of economics at Notre Dame and the failure of it?

Well, I think one should be somewhat pragmatic, give university administrators some of the things they want, and be reasonable with them. What they want is greater visibility and having departments that are respected in academia. Not necessarily becoming a 'top' department, something that is hard, if not impossible, to do. Furthermore, we should avoid internecine fights within the heterodox camp. Being in a pluralistic department or primarily heterodox department, we thought the world was dominated by us; but it was not! Also, one needs to find colleagues who work with mainstream methods, but are tolerant of other views and interested in social justice issues, and who will not be used by administrators who have the power and courage to make big changes but lack vision and wisdom. The other thing is that we were unlucky in that Notre Dame had so much money. The administration could quickly hire a lot of people who emphasize techniques and use mainstream approaches without knowing that there is anything else. So, a lack of money might protect heterodox economists in some cases. Go find places that don't have too many resources! That's not too hard to do.

How do you see the future of post-Keynesian economics?

I know post-Keynesians shouldn't predict. But we should take care of institutional issues, like associations, journals, meetings, and summer schools, and so on, which can be called post-Keynesian. We should be doing our own work on institutions and power and how they change over time in an uncertain world (Dutt 2015). If we do these things, there is no reason for post-Keynesian economics not to flourish. But personally, I don't like that word. Why name an approach after a person, especially a dead, white, man from the UK? Although Keynes is a great scholar who is worth studying, there is too much hero-worship. Also, why just 'economics'?

What do you prefer? Political economy?

Yes, political economy or socio-political economy. Now, as one of the editors of the *Review of Social Economy*, I find that society, too, is important. There is a need to demolish artificial barriers that separate individual disciplines such as economics, sociology, and politics. Socio-political economy needs to do that, without being wedded to a particular approach. Still, I think institutionalists, post-Keynesians, Marxists, feminists, social solidarity advocates, and others should have their groups but there should be more connections between them and connections at different levels. I would argue that one of the things that have really intellectually weakened orthodox economics is this preoccupation with methodological individualism and optimization, as well as its dubious view of rationality, a concept whose meaning is not at all clear, and also its fascination with 'sophisticated' techniques for their own sake. Yes, we have to find simple teachable models. But we also have to understand that there has to be some framework of analysis which goes beyond the simple orthodox view of the world. So, I think we need to be genuinely pluralistic in attitude in terms of epistemology and methods (Dutt 2014), but continue doing our own work to focus on social injustices. And who knows, maybe we'll always be heterodox (opposing the concentration of power, which is invariably harmful) and never the orthodoxy. But maybe, at least, we can protect some bastions where we can flourish.

Any recommendations for the young generation of heterodox or post-Keynesian economists?

During my years in the economics department at Notre Dame, I told young heterodox students that it's going to be hard for them to get jobs. However, I was wrong, because

many colleges prefer a teacher who can provide a broad education, somebody who can talk about many interesting and important issues besides just some optimization models and some sophisticated techniques. The jobs are there, but our students might be ashamed to have a job at a college, and not be at Princeton or Harvard. But why is it important to be in these places, anyway? And they can get other jobs at other departments, at good colleges in- and outside the US, and at policy institutes and research centers. Maybe they can also work on policy studies, international studies, and development studies. So, I think there is going to be a future for heterodox economics. Indeed, there needs to be one, not just for us heterodox economists, including post-Keynesians, but also because I believe that heterodox economics is very much needed for the health of economics as a whole, otherwise it's just going to become increasingly irrelevant for understanding the real problems of the real world.

Okay, thank you very much.

This interview was conducted by Eckhard Hein and Marc Lavoie in Berlin in July 2019, during the 7th FMM International Summer School on Keynesian Macroeconomics and Economic Policies. We thank Sophie-Dorothee Rotermund for the transcription.

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